

UNIVERSITY OF ILLINOIS  
DEPARTMENT OF BACTERIOLOGY

November 12, 1951

Dear, Al, Joshua, Mark, and Max (and respective cohorts):

I imagine you have received a copy of a MS (in press) by Lwoff including a proposed nomenclature of bacteria and phage according to their mutual relations. Bertani and I, after discussion at our seminar, have summarized our objections and criticisms in the enclosed pages. Page 0 comes to you only. Pages 1 - 3 are sent also to Lwoff.

I should make it clear that we feel strongly, not only against the proposed system of nomenclature, but **against** any such system at this time. We see no need for it, no use in it, and several dangers.

Cordially yours,



S. E. Luria

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Comments on proposing the nomenclature.

One of the outstanding features of phage work from 1940 till 1951 has been its cooperativeness and the absence of deliberate attempts to "scoops," priorities and "ownership." Questions of nomenclature, coordination of publications and the like have been handled by consultation and agreement. We dislike the policy of forcing a nomenclature as an accomplished fact, by publishing it before asking for comments and announcing one's intention to use it, irrespective of any comments.

## I. General comments on the nomenclature.

We have two main objections: first, the nomenclature is restrictive and formalistic rather than flexible and adaptable. Second, it attempts to define bacteria as such and phage as such in term of situational interactions between phage and bacterium. Thus, a bacterium, as we shall see below, can be sensitive or immune, euphagic or dysphagic towards the same phage in different phases of its life cycle (for example, B towards T2, depending on whether B has previously been infected with another phage or not, or whether T2 was irradiated with UV, x-rays, etc.). "Situational" definitions are dangerous because they confuse the reader as to the extent of their validity and tend to make the property described appear as an intrinsic one instead of an interactive one. The best example is that of "virulent" versus "temperate" phages. The definition of virulent as "absolutely incapable of lysogenic condition" is bound to collapse in most cases upon thorough search, and is contrary to the biologically sound presumption that every existant phage is carried lysogenically in nature. An example in point is that of phage P2, carried by E. coli lisbonne and Carrere: on the one hand, P2 on sensitive Sh. dysenteriae lyses most cells and establishes lysogenesis on a few; on the other hand, the mutant P2h<sub>B</sub>, active on E. coli, does not establish similar lysogenesis on this host.

The disadvantages intrinsic in any supposedly rational new system of nomenclature are illustrated in the abandonment of the clearly definable, well-established term "lysogenic" in favor of "merophagic."

## II. Specific criticisms.

1. The "non-receptive" term is ambiguous, in view of Garen and Puck's results, which show some "resistant" bacteria to be blocked in step 1 (reversible), some in step 2 (irreversible adsorption). The definition of resistance as given in the Syllabus (Viruses 1950) is much ~~more~~ preferable for the time being.
2. As mentioned under General Comments, the distinction between euphagic, dysphagic, and anaphagic collapses within the course of infection. Moreover, there seems to be little use for a nomenclature according to which a bacterium is euphagic for T4, dysphagic for UV-T4 and immune (anaphagic ??) for X-rayed T4. Similar objections can be raised concerning B + T5 in absence of Ca<sup>++</sup>. These examples illustrate better than words the pitfalls of a method based on designing the reactants in term of the end results of one specific reaction.
3. The supposed correlation between UV sensitivity and virulence or temperance of phages (footnote 3), based apparently on two cases, does not hold for the phages P1, P2, P3 of E. coli Lisb. and Carr. They are about as UV sensitive as T4. See also General Comments on "virulence and temperance."

4. The admission made in footnote 4 negates by itself the presumed absolute nature of the distinction between "virulent" and "temperate" phage.
5. Where would a case be placed, in which a bacterium permits the development but not the full maturation of a phage? Such cases exist in Luria and Human's work on E/4 mutants and are quoted to show how a nomenclature of this type is restrictive rather than heuristic.
6. The abbreviation system is probably the most dangerous aspect of the proposal, since it is the one most likely, if used in forthcoming French publications, to generate long years of confusion.
  - (a) The fact that any "virulent" phage is likely to be ultimately found to be "temperate" will cause changes in nomenclature, whose dangers are well known to geneticists (See the  $\alpha$ - $\beta$  T1-T2 confusion).
  - (b) The capital vs. lower case system is inapplicable to numbers and to several Greek letters, without generating confusion.
  - (c) The principle of genetic nomenclature, by which the symbol should as far as possible describe the genotype rather than the previous history of an organism, is openly flouted in the proposed nomenclature for the bacterial hosts.
  - (d) Priorities, like that of using parentheses for lysogenically carried phages (see William Smith, J. Gen. Microb., 5:458, and Bertani, P.I.S. #6) are disregarded without reason.

We are more opposed to the proposal for abbreviations, especially insofar as it concerns phages, than to the nomenclature itself, which is likely to fade away in any case, like most restrictive systems of definitions.